October 31, 1950.

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Dear Kim-

Your letter received yesterday AM, and I was glad of it too, because the buzzard you mentioned walked into the lab at about 4:30 yesterday afternoon. He seems a very interesting fellow, and I hope we'll see a good deal of him while he is here.

I wish I could scold you for starting a rumor that I had the C1-c2 strains, but it's too funny. Tulasne completely misunderstood my request for Boivin's strains, and at Madison I was discussing with Latarjet whether he wouldn't do some detective work in Paris to look for them. Yours was the fourth request I had in one day about them!

As to DNA in diploid coli, I've been talking a bit with Hans Ris about it. The main catch is in interpretation: the cells are so variable in number of nuclei per cell, and many of them are clearly mixoploid, that I don't think it would be feasible, as yet, to relate assays in terms of nuclei. But more immediately, these cultures are extremely tricky to work with. If Mrs. Leuchtenberger would care to spend a week here, I'd be glad to make room in the lab, but I frankly think that that would be the only way to learn how to handle them. How about haploid and diploid yeast: has anything been done with that material, which would be far more suitable?

Peggy Lieb has been talking and writing to me about working here possibly in 1951-52. I had to put her off for the current year because we simply did not have space, but there are some hopes for amelioration next September. Of course, I think that Peggy should certainly be looking for a regular position, rather than trying to piece out her future in fellowships, but aside from that, I would appreciate your confidential evaluation of her laboratory personality and her reasoning power. Her performance at Columbus was somewhat of a comedown, but I'm not sure whether this was her innate quality, or the problem she was working on. Even with our expansion, we'd have to squeeze to take her, and I'm a little tired of the passable but unbrilliant quality of the crew I'm stuck with now. What would you think?

We were shocked to hear of Roger's troubles. He was an awfully goodlooking young fellow the once or twice I met him.

Our summer in Berkeley (and en route) was colorless compared to Ryan's flamboyant experiences, but we had a good and restful time nonetheless. We got to know Roger Stanier somewhat better, and Had a very good time with him.

I don't know if you want any comment from me about your local conferences, but I don't see what harm it would do to let Demerec know that he is welcome. It is doubtful he would attend very often if he is not given the chance to run the show, but I never could getvused to the idea of an exclusive club. If I was at all cagey about the Midwest affair, it's only because Aaron and Sz. are running it and putting up some of the funds. If anything, Davis is asking to be excluded on the grounds of obstreperousness (don't take this seriously!)

Esther and I are, or are going to be, very much amused at Ryan's travelogue. I don't know whether the department could make a better profit by publishing it, or by using it for blackmail. By the way, ware the Bufo alive on arrival?

I have lately been screening a large number of coli isolates for crossability with K-12 by a combination nutritional-inhibitor selection method: a K-12 derivative which is X-S^r is plated with a wild isolate, X+S^s on streptomycin minimal, which selects for X+S^s recombinants (and rare S^r mutants). This works beautifully with K-12, but some 50 other isolates so far tested do not cross with the K-12 X-S^r in measurable yield, with one exception that I had had some previous evidence for intercrossability. This one crosses extremely poorly, but I have a number f-1 isolates which might backcross more readily to K-12. The new strain is sucrose+ and produces a colicin for K-12, andI'm trying to extract these genes and transfer them to K-12. Some very funny,(but too confusing to report) things are happening with the colicin. I've also had some indication of transgressive segregation, some of the f-1s fermenting sucrose more intensely than either wild type parent. One of the puzzling things about these crosses is the very tight linkages between unselected markers. I'm wondering whether the "recombinants" may not be some more complex gene exchange (insertions?) than simple crossing over. They are not simple heterozygotes, stable or unstable, because their is no preferance for dominant phenotypes.

Best to all the Atwoods,

Sincerely,

Joshua.